THURSDAY, APRIL 27, 1893.

DYNAMICS IN NUBIBUS.

Waterdale Researches; Fresh Light on Dynamics. By "Waterdale." (London: Chapman and Hall, 1892.)

WHEN St. Paul tried to convince the Athenians that they were mistaken in their philosophy, he probably spoke to them in Greek instead of expecting them to learn Hebrew. "Waterdale" is trying to convince nineteenth century philosophers that it is possible to invent mechanism by which he can attain "the undoubted theoretical possibility of *perpetual motion*," and he does not take the trouble of learning the language of those whom he desires to convince, but insists that they must learn his language, simply because he *professes* to have invented a possible explanation of gravity. He acknowledges that his work would require at least a month's hard work to comprehend, and taunts the scientific world for not gladly spending this time in refuting what most of them have already spent weeks on—namely, refuting the very ingenious inventions of cranks, who think to cheat nature in the dark by some roundabout way of doing what simple considerations show to be impossible. A good month's work to teach him! Let him pay somebody with a reputation whose time is probably worth twelve hundred a year, say a month's time, one hundred pounds, to explain and convince him of the impossibility of his mechanical arrangement. It would take more than a month, however. If human experience is worth much it proves that there is very little use in trying to convince people with missions whether they are right or whether they are wrong. And fortunately so; for, if they are right they will ultimately prevail, and if they are wrong after all they generally do more good than harm by interesting the world in something outside and better than the selfish interests of individuals.

"Waterdale" attributes a good deal of importance to this mechanism. He says in his preface: "Let the scientific reader, I would ask, take the trouble first to go through these calculations, and he will then have some idea as to whether the rest of the book is worthy or not of careful perusal." In the body of the work he invents a very complicated hydrodynamic machine to effect his purpose. He there refers to the very much simpler arrangement described in the appendix, and says: "Unless the possibility" (of perpetual motion) "is admissible, then I must confess that the theory of equal real ponderosity to all matter can *never* be accepted." He acknowledges at the same time "that with full knowledge of the liability to error when dealing with the action of forces," all he can reasonably do is to ask "that . . . pure mathematics be once more applied to the subject." All the same, he asserts that "no disproof can be, or has up to the present been given." "There is no speculation about this, but simple fact, if calculation by figures can be accepted to be true." There are so many things touched on in the work that do not seem in any way necessarily connected with the question of "equal real ponderosity," that it is desirable to show how much interest "Waterdale" feels in this part of his theory in order to justify the paying of any serious attention to what can, on general principles, be so easily disproved. It would certainly not be worth while investigating the question in a scientific journal in order to convince the author of the paradox. He could only be convinced by very painstaking and judicious personal interviews of his error and of the unimportance of this question of equal real ponderosities. It would hardly be worth while investigating the question merely because "Waterdale" attributes importance to it, but it is worth while doing so because others may attribute importance to it, and still more so because "Waterdale's" mechanism is interesting and involves a principle that is intimately connected with the second law of thermodynamics, Boltzmann's hypothesis, and a lot of recondite questions which are puzzling the scientific world, so that it is not much wonder that even a clever and ingenious person should get involved in its meshes, especially when that person is involved in a "mission."

The general idea involved in "Waterdale's" mechanism is as follows:—Suppose a large body (he objects to the word "mass") M and a small one m, and a spring or other means by which kinetic energy can be given to the bodies. If the spring exert a constant force F through a space s_1, it would communicate a velocity V_1 to $(M + m)$, given by the equation—

$$Fs_1 = \tfrac{1}{2}(M + m)V_1^2.$$

If now it work through a distance s_2 it will increase this velocity to V_2, when

$$Fs_2 = \tfrac{1}{2}(M + m)(V_2^2 - V_1^2).$$

So far all is plain sailing. But we may proceed in another way. We may let the spring work against m alone, and then by suitable mechanism use m's kinetic energy to make the combined system $M + m$ move. In this way we might expect to give m a velocity v_1, such that $Fs_1 = \frac{1}{2}mv_1^2$, and when this energy was spent on the two bodies $M + m$, they would acquire a velocity V_1 the same as before, given by $\frac{1}{2}mv_1^2 = \frac{1}{2}(M + m)V_1^2$. Now comes an important assumption, that if the *relative* velocity of m and M be equal to v_1, then by proper mechanism it must always be possible to *increase* M's velocity by V_1, while m's velocity is being reduced to V_1.

Suppose now m_1, moving with velocity V_1, we act upon m by means of the force F, again through the distance s_2 we have for its final velocity v_2—

$$Fs_2 = \tfrac{1}{2}m(v_2^2 - V_1^2).$$

Hence the relative velocity of M and m is $v_2 - V_1$. By choosing $s_2 = 3s_1$, we can arrange that $V_2 = 2V_1$, as it simplifies the further argument. In this case

$$v_2^2 - V_1^2 = 3v_1^2 \text{ or } v_2^2 = 3v_1^2 + V_1^2;$$

$$\therefore v_2 = \sqrt{3v_1^2 + V_1^2},$$

and the relative velocity

$$v_2 - V_1 = \sqrt{3v_1^2 + V_1^2} - V_1,$$

which may be much greater than v_1, if v_1 be much greater than V_1, *i.e.* if m be much smaller than M. This shows that the *relative* velocity after the second blow may be much greater than after the first, even though the two blows were so chosen as that if applied directly to the combined body they would produce equal increments of velocity in that body. Assuming then that a given *relative* velocity can always produce a given *increase* of velocity in the combined system, it appears by our assumption that, as the *relative* velocity is much greater after the second blow given to m than after the first, the *increase* of velocity of the system produced by this indirect method of applying the second blow will be much greater than by the first, and consequently much greater than the velocity that could be given to the system by applying the blow directly. By reducing the system to its otherwise produced velocity V_2, we could obtain a certain amount of energy, and then repeat the process *ad infinitum*, thus obtaining a continual supply of energy.

An investigator without a mission would be led by this curious result to assume that there must be some mistake in his arguments, and "Waterdale" evidently has some lurking doubts. He sees that it is impossible in the simple case of bodies having only one direction of velocity. Impact can never reduce two bodies of a system to move with the same velocity *and* conserve energy. We cannot have momentum and energy both conserved. Unless M = o we cannot have

$$mv_1 = (M + m)V_1$$
$$\tfrac{1}{2}mv_1^2 = \tfrac{1}{2}(M + m)V_1^2.$$

In order to divide the energy $\frac{1}{2}mv_1^2$ between the two bodies and reduce them both to a common velocity, we require a *third body*, and then what becomes of the principle that seemed so plausible, that the increased velocity that m could impart to M depended on their *relative* velocity only? "Waterdale" sees the hitch all right in the simple case, and consequently, in order to cheat nature by inventing a complicated case in which he hopes that she will get as muddled as himself, he interposes bent channels, a third and fourth body to receive the blows, springy arms to absorb energy, and smooth surfaces to divert the motion. He evidently has some doubts about all this, for, notwithstanding his assertion that "Appendix II. is a mechanical demonstration to prove that by the principle of *velocity* of force, a saving in mechanical work, . . . can be effected," and that "there is no speculation about this, but simple fact," yet he gives only a series of suggestions and vague estimates as *unspeculative proofs*, that the energy spent in bending his springs, in jumping his bodies about, and so forth, is negligible, while in reality it is an important part of his system. That it is so necessarily is proved conclusively by the impossible result he obtains by neglecting it. This is the really interesting principle in the whole matter, that it is not possible to give energy to a system of bodies by giving a series of impulses to some particles of it, to be transmitted to the rest of the system by actions within the system without some part of the energy being spent on internal motions in the system. It is here that the example touches upon the second law of thermodynamics, Boltzmann's hypothesis, and so forth. In order to minimize the effects of these internal vibrations, &c., "Waterdale" argues thus: "Loss No. 2" (giving rise to internal vibrations of his system) "if it arises" (he himself shows that it would, though he overlooks a more important loss), "would be of the nature of internally asserted work." . . . "This loss of work could not be great, for we see by the diagram that the span of work already done when the ball arrives at o is small compared with what it has to do." Notwithstanding his profession of calculating everything he does not calculate here, nor does he calculate with what velocity the ball would rebound after it hit the body B, which ultimately stops it; in fact he omits this important question altogether, and goes to the "third factor, the bending of the arm of the system," which he goes on to say, *without calculation*, "can be almost neglected if we take the tension of elasticity of the arm to be small." "I should say that one-eighth internal loss of work would certainly more than cover everything." This blessed "I should say!" Is it thus that "Waterdale" gives "a mechanical demonstration to prove . . . . a saving in mechanical work"? "There is no speculation about this"! It is "simple fact, if calculation by figures can be accepted as true." Most people would agree that "if calculation by figures can be accepted as true" the velocity that could be given by *any* mechanism to the system indirectly could not be greater than what would give it kinetic energy corresponding to the work supplied. If "Waterdale" will apply a system of levers, springs, &c., acting on the fixed bodies of his system, so as to reduce all the bodies to relative rest, and thereby gives up as hopeless the task of inventing some method by which he can by internal actions alone transfer kinetic energy from one body of a system to the whole of the system without wasting any of it in internal kinetic or potential energy, then he will see how he has to give up the apparently legitimate assumption that the velocity one body of a system can give to the whole system by being itself reduced to relative rest depends *only* on the *relative* velocity of the body and the rest of the system. He will see that it depends also on the velocity of his system relative to those supposed fixed bodies he will require as fulcrums for the mechanism required to transfer the energy of the one body to the rest of the system. He sees that something is required to keep his wedge moving forward. He arranges "that the wedge is supported by a following force . . . during this part." The amount of work required he without calculation *assumes* to be small, and he is probably right here; but it is only one of several losses that he does not *calculate*, and there are others, such as the conditions of impact at the end of the flight of m, that he does not even notice, though this is the very first that should strike a person investigating the subject after he had clearly seen, as "Waterdale" appears to do, that it is here, in the laws of impacts, that the simple case of velocity in one direction and direct impacts fails. It is interesting how cases of this kind illustrate the warming of a gas by compression, the vibrations produced in a bell when struck, and other such cases where energy is given to one part of a dynamical system for this part to distribute amongst the whole, and also how it illustrates the way in which the amount of this internal energy depends on the mobility of the part originally moved. Of course it is all plain enough when the subject is attacked by means of general principles of conservation of energy and momentum, but when the interactions of the different parts of the system are individually considered and the mind distracted by the complexity of the problem, there is real danger that what is important may be overlooked as trivial, as has been done by "Waterdale." He is not to be blamed for this, but he is to be blamed for putting forward as a proof in which "there is no speculation," as a "simple fact, if calculation by figures can be accepted as true," an investigation in which he acknowledges that he estimates this, that, and the other without calculation.

And after all, what does he require all this elaborate attempt to cheat nature by complex mechanisms for? Simply because he does not understand fully the position of scientific men in respect of the word "mass," and because he has some *à priori* difficulties in his own mind as to how atoms of different masses can require equal quantities of heat to warm them through equal ranges of temperature. He *says* that scientific men say that *because* a cube of gold weighs seven times as much as a cube of aluminium, "it is therefore taken to comprise seven times the quantity of matter; *ergo* it possesses seven times the attractive force, and falls with equal acceleration; *ergo* also it requires seven times the force or work to move it." Now this is a gross libel on scientific men. That it requires seven times the force to move a cube of gold that is required in the same time to generate the same velocity in an equal cube of aluminium is a matter of experience, and is the only reason why it is said that the mass of the gold is seven times as great as the mass of the aluminium, and this is said because the statement is only using the word mass in accordance with the definition of the word. That there is *therefore* seven times the quantity of matter is really no question of *therefore*, for the statement is again merely a definition of the term "quantity of matter," which is, in its scientific use, only another name for mass. Now come questions about gravity, and as no satisfactory explanation of gravity (*pace* Le Sage, Tolver Preston, Osborne Reynolds, "Waterdale," and a host of other theorisers upon this interesting subject) has yet been propounded, no scientific person can rightly say that a body attracts seven times as much as another *because* it has seven times the mass or quantity of matter, for until we know the cause of the attraction we have no right to say that it is *because* of this or that. Hence there is no *therefore* at all put forward by scientific men between "seven times the quantity of matter," "or mass," and "seven times as heavy," or "seven times the attractive force." That a body with seven times the mass of another does as a matter of fact weigh seven times as much is a matter of experience, but that it does so *because* it has seven times the mass is a mere conjecture, and that it is so held by scientific men is proved by attempts having been made to prove by experiment that weight is proportional to mass, and even to find whether weight varies with the direction of the axis of a crystal, &c. "Waterdale" objects to supposing the elementary atoms bulk for bulk to be of equal density, because "we should have to place the atoms in a light substance too far apart," a fairly good reason for investigating the question though not for deciding it. On the other hand he objects to supposing "each atom to be more or less porous—a very incredible hypothesis"—for reasons depending on specific heats to which he evidently attaches some weight, as he harps upon it more than once. Why he should think it so incredible that the atoms may be porous does not clearly appear, for his own atoms, as described in the book, are eminently porous, and it is upon their porosity that his whole explanation of their behaviour to force, and his application of his principle of "velocity of force," and his theories of light and electricity and chemical action and adhesion all depend. He would probably reply that he does not want them *so* porous as all that, though this hardly justifies the epithet "incredible" in respect of a hypothesis he himself holds. Anyway, his serious reason for disbelieving in the unequal masses, or, as he calls it, ponderosities, of atoms is the difficulty he has in seeing how equal quantities of heat can raise unequal masses through equal ranges of temperature. His difficulty rests upon his imagination that he understands fully what the wisest men would probably say they did not understand at all fully, namely, on what property of the atoms of a body temperature depends. He discusses the matter pretty carefully. He says: "How are we to account for the apparent fact that the work of a quantity of heat which is equal to raising weight, 1 of water 1° of temperature—or, in other words, to accelerate, it is to be inferred, the vibrative motion of the whole of its parts in the degree corresponding to one more degree of heat—will be also equal to giving equal acceleration to the entire parts of 8·784 and 30·816, respectively, more matter in the cases of iron and gold?" He here assumes that equal increments of temperature correspond to equal increments of "acceleration" of the atoms. This, if it means anything, is not true, and it is not *à priori* at all likely. Take another place, where he says, ". . . . The fact that a given quantity of imparted heat raised a *really* heavy atom to the same temperature as it would a *really* lighter atom, would indicate that equal temperatures were marked by a slow motion of heavy wedges in respect of a heavy atom, and by a quick motion of light wedges in respect to light atoms."

"Although the same quantity of heat might thus be imparted to the two atoms, it is reasonable to infer that the intensity of the heat, as made apparent to our senses, would not in the two cases be identical." ". . . It is reasonable to infer," on the other hand, that there is some hitch in an argument that depends to any important degree upon such a form of expression as "it is reasonable to infer." Is it not, on the other hand, most reasonable to infer that the blow given by a light body moving quickly would be very much the same as by a heavy body moving more slowly, and that, consequently, the "intensity of heat," as he calls it, would feel the same? In any case a very cursory study of the kinetic theory of gases would point out how there is certainly no incongruity or incredibility, but quite the reverse, in the notion that equal quantities of heat do make light atoms move rapidly and heavy ones slowly; and that, notwithstanding their different atomic velocities, the temperatures of two bodies may be the same. There is no real difficulty in supposing that it requires thirty times as much heat to raise the temperature of water as is required to raise an equal mass of gold through the same range of temperature, if we bear clearly in mind the very complex structure of both water and gold, and all that has to be done by the heat in each case, and at the same time recollect how very little we know of the conditions that determine when two bodies are at the same temperature, *i.e.* that determine that on the whole no energy shall flow from one to the other when placed in contact or radiating to one another.

There are many other matters treated of in the book, but if one were to take "Waterdale" at his word and judge "whether the rest of the book is worthy or not of careful perusal" by one's experience of Appendix II. and its supposed proof, nobody would read another word, and unless one had a great deal of leisure to devote to speculative conjectures, or were well paid for it, there does not seem much inducement to wade very carefully through it. "Waterdale" professes to explain gravitation by a sort of hotch-potch of Bjerknes' sound wave attractions and Osborne Reynolds's theory founded on dilatancy. He seems to think that any attempt to explain gravitation is very remarkable. "The author would have thought that when the unusual occurrence of the publication of a work announcing the discovery of gravity and other original theories as important arises, that the scientific world would display sufficient interest in the subject as to read and examine the arguments, although the work might be by an unknown pen." "Waterdale" seems ignorant of the fact that the scientific world has been inundated with theories of gravity and other original theories. To mention only a few of the better known ones, there are Le Sage's corpuscular theory, worked out very carefully by Mr. Tolver Preston and Mr. George Forbes. Others founded on wave motion and fluid flow, such as Bjerknes has popularized, and which Mr. Karl Pearson has devoted so much ingenuity to, though he takes refuge in nondynamical suggestions, such as a fourth dimension, which might just as well be introduced as a region in which a convenient series of strings existed to hold atoms together without any action at all going on in our stupid tridimensional space. What the difference is between such a theory and the good old hypothesis of inherent qualities seems difficult to discover. Then there is the suggestion that every atom is connected to every other by means of vortex filaments, though how the poor things work when they get tangled is rather a difficulty here. Finally, there is Osborne Reynolds's interesting theory founded on dilatancy, which very possibly has a future before it, especially if we consider that the ether is probably full of vortices, and that vortices cannot cut one another. These theories almost all suffer from the apparently incurable defect to which "Waterdale's" is also liable, that they give a rate of propagation of gravity comparable with that of light. Parents are proverbially partial to their children, and "Waterdale" probably will cherish his suggestions as very valuable, notwithstanding this and other serious objections. The confident way in which, after pages of suggestions as to what might happen, he states that a current from right to left will produce one effect, while one from left to right will not neutralise it is quite refreshing, but is not an attractive investigation to those who are accustomed to call nothing a proof that is not founded upon something better than suggestions. That gravity is propagated with such amazing rapidity as it is seems to show that it must be an action of the medium to whose *structure* the electromagnetic properties of the ether are due. Such actions are known to exist in a perfect liquid, and it is natural to attribute gravity to such actions. The reasons for attributing great velocity of propagation to gravity are not apparently very well known. The difficulty is owing to the component of the force at right angles to the radius vector that would come in, owing to the aberration of the force, and which would cause an acceleration of areas of planets. This might be partly neutralized by a resisting medium, but hardly completely, especially in the case of comets, because the resisting force would be tangential to the path, while the aberration component would be at right angles to the radius vector. It is possible, by assuming an increase of force due to velocity of approach and a decrease due to recession, to get over this latter difficulty; but even then it is hard to explain the persistent rotation of the earth when the surface is not moving freely as a projectile, and when consequently the supposed exact balance between gravitational acceleration and resistance of medium does not hold. Even then there is the possible suggestion that cohesional and other forces, being similarly propagated in time, would prevent any possible effect being produced by the resisting medium, and so matters return to much as they were at first, and no final answer be given to the questions, "Is gravity propagated in time?" "Does the ether offer resistance to motion?" It remains much in the same position as the question of the motion of the ether at the surface of the earth.

"Waterdale" and others seem to think that fluidity necessarily implies that a medium is divisible into hard or soft particles. No ordinary mind is forced to this conclusion. Most minds look upon water, for instance, as a perfectly continuous medium, any part of which can flow past any other part with perfect freedom. Hardness, softness, and so forth may require structure, but mere fluidity does not. Again, "Waterdale" and others seem to imagine that elasticity essentially involves the *compressibility* of the elastic body: *i.e.* that it must consist of atoms that are themselves compressible. "Waterdale" himself invents a structure for an atom that resists *deformation* without its constituents being themselves compressible, and the existence of vortex rings shows how a perfect liquid can have a real elasticity to deformation given to a part of it by giving it motion without any part being composed of particles, or any part of it being at all compressible.

The rest of "Waterdale's Researches" concern suggestions as to how cohesion, chemical action, light, electricity, &c., may at some future time be explicable by the structure he proposes for the ether, which is to all intents and purposes the same as Osborne Reynolds already has suggested, a whole collection of absolutely hard bodies of different sizes, or, as "Waterdale" suggests, spheres of two different sizes. There is considerable cleverness displayed in the way he has reasoned out for himself such a well-known theorem as that a body moving in a perfect liquid will behave as if its mass were increased, but the labour bestowed upon such a well-known theorem does not entice the reader to try and follow the vague suggestions that follow, and that are much the same as have been over and over again given to show how *every* theory as to the nature of the ether explains a lot of things which can on the face of them be explained by *any* ether through which bodies can move, and upon which they exert pressures. Mixed up with these plausible suggestions are such things as hypothetical whirls of ether within the solar system that seem, to say the least of them, to require some elucidation as to how comets go through them in every sort of direction without any sensible action of the whirl on the comet.

A person who has brought forth, after enormous labour of thought, a series of theorems concerning the universe, and who is not very familiar with the equally carefully thought-out suggestions of others naturally looks with more favour upon his own children than upon those of others; but, if he is reasonable, and in a reasonable mood, he will not be surprised nor even distressed, because those who look at all these children with critical eyes see very serious defects in all of them, and feel very confident that without great changes no one of them can possibly grow into a second Newton.

VERTEBRATE BIOLOGY.

Text-book of Biology. By H. G. Wells, B.Sc. Lond., F.Z.S. With an Introduction by G. B. Howes, F.L.S., F.Z.S., Assistant Professor of Zoology, Royal College of Science, London. Part I. Vertebrata. (London: W. B. Clive and Co., University Correspondence College Press.)

MR. WELLS'S book is avowedly written mainly for the purpose of helping solitary workers to pass the Intermediate Science examination of the University of London, and it would therefore be unfair to criticise it from a wider point of view. The scope for originality in such a work is naturally somewhat limited, but it is a pleasant surprise to come across one which is far above the average as regards soundness of treatment and method. The author not only possesses a practical knowledge of the greater part of the subject he deals with, but also evidently takes pleasure in it for its own sake, and has a healthy dislike of "that chaotic and breathless cramming of terms misunderstood, tabulated statements, formulated 'tips,' and lists of names, in which so many students, in spite of advice, waste their youth." He states that "the marked proclivity of the average schoolmaster for mere book-work has put such a stamp on study that, in nine cases out of ten, a student, unless he is expressly instructed to the contrary, will go to the tortuous, and possibly inexact, description of a book for a knowledge of things that lie at his very finger-tips" (p. 31); and again, on p. 125, that "it is seeing and thinking much more than reading, which will enable" the student "to clothe the bare terms and phrases of embryology with coherent knowledge." Throughout the book the importance of actual observation is insisted upon.

The present part deals with the Rabbit, Frog, Dog-fish, and Amphioxus, and includes an account of the development of these animals and of the theory of evolution, as well as a number of questions, most of which have been set at the examinations of the London University. The morphological portions are, on the whole good and clearly written, and a fair amount of physiology is also introduced. A syllabus of practical work is given at the end: this would in many respects bear amplifying. The student is not warned that his time will be wasted if he wanders off the direct path of the examination syllabus; and on the contrary, points of general biological interest are referred to here and there, and these go far to show what a good many of our elementary text-books do not —viz. that the London University syllabus, "as at present constituted," affords "considerable scope for efficient biological study." The student, moreover, is told that this "little book is the merest beginning in zoology," and the last paragraph, on p. 131, indicates the aspect of mind with which the author regards his subject.

Twenty-four folding sheets of sketches are inserted in the text, but the figures are, on the whole, exceedingly rough; and though many of them may be found useful as guides, we feel that the student would do better to postpone drawing until his dissections are made, or even copy some of the numerous good figures to be found elsewhere, than to "copy and recopy" these sketches first, as advised by the author.

Numerous inaccuracies and awkward expressions occur, only a few of which can be here mentioned. The terms superior and inferior, as applied to the great veins, are likely to confuse a beginner after reading the definition of the regions of the body given on p. 3. "Metabolism" and "metaboly" occur even in consecutive sentences on p. 23. Peristaltic movement is said to move the food "forward" (p. 41). It is stated that the thyroid is similar in structure to the thymus and to "botryoidal tissue" in general (p. 26), and that the epithelium of the villi, with its striated border, "is usually spoken of as leading towards "ciliated" epithelium (p. 22). It is misleading to say that "a tarsus (tarsalia) *equals* the carpus," and that the vomer of the dog is paired (pp. 38 and 76). As the term "Chordata" is adopted on p. 96, it is unfortunate that the student is told on p. 60 that vertebrata occur in which cartilage is absent, and that Amphioxus possesses the "essential vertebrate features," is "*twisted*, as it were," and that its "vertebral column is devoid of vertebræ:" it is, moreover, inadvisable to use the term "hyoidean" with regard to this animal. On p. 61 "classes" and "orders" are used in a correct and an incorrect sense in the same sentence. The expression, "carotid gland" requires a better explanation on p. 67. The morphology of the cardinals, azygos, and post-caval is incompletely explained (pp. 87, 120, and 124). Several serious mistakes are made with regard to the homologies of the urinogenital apparatus (*cf.*, *e.g.* pp. 92 and 114). Misprints are also fairly abundant throughout.

Most of these faults are, however, such as can be remedied in a future edition, and the book will, we think, serve the purpose for which it was written very satisfactorily.

W. N. P.

OUR BOOK SHELF.

Pflanzenleben. Von Anton Kerner von Marilaun. Band II. Geschichte der Pflanzen. (Leipzig und Wien: Bibliographisches Institut.)

THE first volume of this excellent book was reviewed in NATURE, vol. xxxix. p. 507. The present volume, which completes the work, treats of the "history of plants," by which is meant their *development*, in the widest sense, including both ontogeny and phylogeny. The former subject ("origin of descendants") occupies the first 480 pages, while the remainder is devoted to the "history of species."